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In fact, I may sum up the different qualities of the two constructions by saying that, whereas Prof. Adams's is the proper construction if a problem of the kind has to be dealt with *ab initio*, mine is, I conceive, the proper construction for a reference-figure. I repeat, however, that the principle of my construction cannot be regarded as new. Moreover, as a geometrical expression of the relation between the time and the position in elliptic motion, Prof. Adams's construction is manifestly superior.

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## *On the Rejection of Discordant Observations.*

By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

In the *Monthly Notices* for April 8, 1868 (vol. xxviii. pp. 165–168), Mr. Stone suggested a criterion for the rejection of discordant observations, which it was the original object of this communication to examine. Before doing so, however, I wish to allude to a passage contained in a paper of mine on the subject of errors of observation, printed in vol. xxxix. of the *Memoirs* of the Society, in which the mode of treatment to which the theory itself leads is noticed, and to explain this method in greater detail than is there done. It will be seen that it supersedes the necessity for the *rejection* of anomalous observations.

The passage referred to occurs on p. 103, and consists of the quotation from De Morgan, and the remarks that precede it. As the extract from De Morgan is the foundation of what follows, and is very short, I here quote it again:—"Assuming the weights as nearly as they can be found, ascertain the most probable result, from which find the weights of the equations. If these agree with the assumed weights, the process is finished; if not, repeat the process with the new weights, and so on, until a result is obtained for which the assumed and deduced weights of the equations are sufficiently near to equality."

Believing this to be the true mode of completing the treatment of observations (and having been independently led to it), I proceed to develope its principles rather more in detail, and to answer some possible objections.

The problem we are concerned with is, Given a number of direct observations of the same quantity, determine the most probable value of that quantity. This is, of course, not the most general way of stating the question, as it is assumed that only one unknown is to be determined; but its discussion involves all the essential principles, and in a form free from unnecessary complication.

The usual reasoning is to suppose that all the observations

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were made subject to the same law of facility $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, whence the arithmetic mean of the observation is the most probable value of the quantity observed. The value of $h$ is then determined approximately from the observations, and thence the probable error.

Now, to the conclusiveness of this treatment there seems to be the fatal objection that $h$* is assumed to be the same for all the observations; viz. we act as if we had a reason for *knowing* that this must be the case; whereas, in point of fact, we have no *à priori* knowledge about the matter at all, and no *à posteriori* knowledge except such as we derive from the observations themselves. Our only ground for taking $h$ to be the same for all the observations is the total absence of any reason for supposing, *à priori* (viz. before the observations are made), that any particular one will be better than any other; and the knowledge that if the number of observations were *infinite* in number, $h$ would be the same for all.

But when the observations have been made, an examination of them cannot but afford information of some sort with regard to the probable values of the $h$'s of the laws to which they were subject; and it is the information so given that the usual treatment ignores—an omission which gives rise to more or less arbitrary rules for the exclusion of certain of the observations, a clumsy expedient at best.

To state the matter more mathematically, if $V_1, V_2, \ldots V_n$ be the observations, the usual treatment gives for the most probable value of the quantity observed, $a$ the arithmetic mean of $V_1, \ldots V_n$ and a certain value of $h$, say $h_0$, so that all the observations are supposed to have been subject to the law $\frac{h_0}{\sqrt{\pi}}e^{-h_0^2x^2}$; but, knowing that there is no necessity for the $h$'s to be the same for all the observations, we see that the conclusion contradicts the premises, if we stop here; in other words, that $a$ is only a first approximation; for $a$ being a value very near the truth, then the errors of the individual observations are very nearly $a-V_1$, $a-V_2$, ... $a-V_n$ (which call $v_1, v_2, \ldots v_n$), and it follows therefrom that $V_i$ ($v_i$ being supposed the numerically greatest of the series $v_1, v_2 \ldots v_n$) was most probably subject to a law in which $h$ was less than the $h$ of any other of the observations, and so on for the others. Thus the knowledge that $a$ is in all probability very near the truth gives us the means of estimating and comparing the $h$'s of the different observations; and we should proceed to weight them accordingly, and so to obtain a second approximation.

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* I shall speak throughout of the $h$ of an observation or a series of observations, and not of the corresponding probable error, as in contemplating an observation, the thing present to the mind is the law of facility $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ which more directly involves $h$ than the probable error $\frac{\cdot 476936\ldots}{h}$.

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This is, I apprehend, the kind of reasoning De Morgan had in his mind when he wrote the passage quoted above, having previously remarked that "we cannot refuse to allow that the establishment of a most probable result places us in the circumstances . . . . where we have to return upon our premises with new probabilities forced on us by the conclusion." (*Encyclopædia Metropolitana*, p. 456.)

The following investigation (in which, for clearness' sake, the process is developed quite from first principles) shows, I think, very plainly the way in which the weighting should be effected.

To fix the ideas, let an observation be regarded as accurate if it differ from the truth by an amount less than a fixed infinitesimal quantity $k$; then the chance of the accuracy of an observation subject to the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ is $\frac{hk}{\sqrt{\pi}}$ and therefore varies as $h$, while the weight varies as $h^2$; so that the weight of an observation varies as the square of the probability of its accuracy.

The data are the observations $V_1, V_2 \ldots V_n$ and the knowledge that the law of facility which the errors follow must be of the *form* $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, and the *quæsitum* is the most probable value of V.

In the first place, assume $h$ to be the same for all the observations; then, if $a$ is the true value of the quantity observed, the *à priori* chance that the $n$ observations should be $V_1, V_2 \ldots V_n$ is proportional to

$$e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}},$$

passing, therefore, from *à priori* to *à posteriori* probability, we have, after the observations are made, the chance that $a$ is the true value proportional to

$$\frac{e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}}}{\int_{-\infty}^{\infty}e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}}\,da}$$

viz. to

$$e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}}$$

so that the most probable value of $a$ is obtained by making $(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2$ a minimum, viz. by taking $a$ to be the arithmetic mean of $V_1, V_2, \ldots V_n$. (So far is, of course, Gauss's reasoning unaltered.)

But at the same time that this argument asserts that $a$ is the most probable result, its probability being

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$$A\,e^{-h^2\{(a-V_1)^2\ldots+(a-V_n)^2\}},$$

it asserts that the probability of $V_1$ being the true result is

$$A\,e^{-h^2\{(V_1-V_2)^2\ldots+(V_1-V_n)^2\}},$$

of $V_2$ being the true result is

$$A\,e^{-h^2\{(V_2-V_1)^2\ldots+(V_2-V_n)^2\}}$$

&c., so that, calling these probabilities $p_a$, $p_1$, $p_2$, . . . it follows that we are told that the weights of the observations $V_1$, $V_2$, . . . are proportional to $p_1^2$, $p_2^2$, . . . . ; and hence for our second approximation we so weight them, and obtain as result the value $b$, where $b$ is such that

$$e^{-p_1^2(b-V_1)^2-p_2^2(b-V_2)^2\ldots}$$

is a maximum. We can thus obtain, as before, new weights $p_1'^2$, $p_2'^2$, . . . ; and the process is repeated until the equations $p_1^{(n)}=p_1^{(n+1)}$, $p_2^{(n)}=p_2^{(n+1)}$ &c. ($p\,a$, $p\,b$ . . . or the probability of accuracy of some one of the observations being always taken as unity, as only the ratios of $p_1^{(i)}$, $p_2^{(i)}$ . . . are involved) are sufficiently nearly satisfied when the process is concluded, and then the best weights of the observations, and by consequence the best value of the quantity observed, have been obtained.

This seems the complete method of treatment of observations by the Theory of Errors, and it will be observed that the only assumption that has been made is that any error follows a law of the form $\frac{h}{\sqrt{\pi}}\,e^{-h^2x^2}$. This, Laplace's proof (when modified, see Memoir previously cited, pp. 104, 105) seems to me to have established; but even if this were not admitted, the law, with the $h$ determined separately for each observation, obviously can be made to represent the facts with a near approach to accuracy.

The calculation of $p_1$, $p_2$, . . . the data of the second approximation presents no difficulty, for $a$ being $\frac{1}{n}(V_1+V_2\ldots+V\,n)$,

$$(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2=n\,a^2-2\,a\,.\,n\,a+\Sigma V^2;$$

so that the probability of $a$ being the true result is $A\,e^{-h^2\{-n\,a^2+\Sigma V^2\}}$, which call unity; so that the probability of any other value $x$ is

$$A\,e^{-h^2\{n\,x^2-n\,x\,a+\Sigma V^2\}}\div A\,e^{-h^2\{-n\,a^2+\Sigma V^2\}}$$
$$=e^{-n\,h^2(a-x)^2},$$

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as can be readily seen otherwise, though not in quite so elementary a manner.

This is, as I understand it, the method advocated by De Morgan; and all that I have done is to state it more definitely and in greater detail.

The sort of objections that I conceive might be raised against the completion of the method of least squares, as just indicated, would be (1) that in a series of observations made under apparently exactly similar circumstances (when obvious mistakes are expunged) we are bound to take $h$ the same for all, and (2) that a kind of reasoning in a circle is involved in making the observations point out their own weight. To the first objection (which might also be enunciated in the form that $h$ must not be altered unless there are grounds for believing in the existence of a disturbing cause affecting some of the observations) the answer is, that in our total ignorance of the *exact* way in which any one particular error does arise, we have not the smallest right to assume, except tentatively, that the $h$ is the same; and this being so, the existence of an observation differing more than another from what we believe to represent the truth very nearly, *ipso facto* involves a presumption that the $h$ for that observation was smaller. The reply to the second objection is, that the reasoning is of exactly the same character as that used in solving an equation or in reverting a series by repeated approximation.

It will be observed that the principles enunciated above are not in the slightest degree at variance with the method of least squares as usually used. I admit the whole of the ordinary treatment; only I go farther, and instead of saying, after the application of the rule, "This is the final result," I say, "No, this is not good enough for a final result, but it is good enough to weight the observations from;" and thence a better result is obtained, and so on.

Of course, if the first approximation is very nearly the best that the observations will give, the process of weighting will alter it very slightly; and it is to be understood that I make no proposition that observations should always be treated in this manner in practice — the work is sufficiently long as at present — but merely point out that the above is the proper mode of proceeding, if more accuracy be required.

I can imagine an objection of this kind: suppose a series of observations were made, and when the $h$ had been determined as usual, they were found to group themselves almost exactly under the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$; would not this be strong evidence that the $h$ was the same for all, and that any further weighting would be an unjustifiable tampering with the observations? To this the reply is that the observations never could exactly follow the law unless they were infinite in number, and that it is the deviations implied in the word "almost" which the weighting would help to

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correct. If the number of observations were infinite, and exactly followed the law, the weighting would make no difference at all.

To consider the matter more fully, if an infinite number of observations had been made (assuming it, *argumenti gratia*, to be possible), and if they were found to group themselves exactly according to the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, then it might be urged that this afforded a moral proof that this was the law, and that the arithmetic mean of the observations was the most probable value. In this limiting case, therefore, we have no occasion to weight the observations, even though some are very far from the truth; so that here the unweighted result is the best possible, and the method cannot rest on a principle universally sound. But the explanation is obvious, viz. that the arithmetic mean is the best result, because every positive error, of whatever amount, is exactly neutralised by a negative error of equal magnitude, so that no additional accuracy would be gained by weighting the observations, as no alteration of the result would follow therefrom. (This limiting case bears a rough resemblance to a series where all the terms become zero except the first.) The same takes place whenever corresponding to every error (or deviation from the arithmetic mean) there is an exactly equal error of opposite sign, as then the *n*th approximation coincides with the first; but in no case that actually arises in practice can it be true that the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ is accurately followed: we may compare the numbeer of rrors absolutely found between 0″·0 and 0″·1, between 0″·1 and 0″·2, &c. with the numbers given by the theory with the assumed value of $h$, and the agreement may be very close, indicating that the arithmetic mean is very near the truth, but this in no way shows that it *is* the most probable result; for in obtaining the arithmetic mean one comparatively large error may have neutralised several smaller ones of opposite sign—in fact, something of this sort always takes place, unless for every positive error there is always an equal negative one—and it does not follow, if an infinite number of additional observations were made under the same circumstances, and the curve of facility (or rather, in this case, the curve of frequency) of the whole, which we know to be of the form $y = \frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, laid down, that then the point where the middle ordinate cuts the axis of $x$ would coincide with the point whose position was merely determined by the condition that the sum of the positive deviations from it should be equal to the sum of the negative deviations in the *finite* series of observations that were first made.

I have been thus particular in noticing this case because it seems to have been sometimes tacitly assumed that because, in a certain number of observations with an assumed $h$, we have a right to expect one or two large errors of a certain magnitude, therefore if these conditions have been realised in a series of ob-

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servations, and if such large errors do appear, we are bound to retain them and give them the same weight as the others; the fallacy consisting in supposing that because such deviations might have been anticipated by the theory, therefore the theory is competent to treat them by itself, the same weight being given to them as to the others.

I may remark that to say the most probable law of facility for the observation $V_i$ was $\frac{h_i}{\sqrt{\pi}} e^{-h_i^2 x^2}$ might seem, at first sight, a little startling, as, such being the case, we have but one observation subject to that law; but it must be remembered that we assume the law to be of the above *form*, and $h_i^2$ is simply the weight.

It is, perhaps, also as well to recall to mind here two things: (1) that Laplace's analysis (though he did not himself so apply it) only proves the law to be of the form $\frac{h}{\sqrt{\pi}} e^{-h^2 x^2}$ if the actual error is a linear combination of an infinite number of smaller errors —this probably represents the facts sufficiently nearly—and (2) that, to adopt Leslie Ellis's terms, the rule of least squares, as usually used, is only the best method among methods, but need not give the best *result* among *results* in the same series of observations: extended as above, it should give the latter also.

From what has been said it is obvious that, if the above treatment were adopted, the rejection of an observation would never be necessary, except in the case where we saw it must be a mistake, and then the knowledge that would enable us to feel certain that this was so would remove the error from the category of errors of observation. An anomalous observation would obtain a very small weight, but never none at all: and this is as it should be. It is for this reason that in my Memoir I expressed my dissatisfaction with Pierce's criterion, viz. because I thought the principle of rejecting an observation *in toto* (except in the case of an obvious mistake) unsound, not because I thought the criterion itself specially objectionable. If we leave the strict course of mathematical procedure, we lose ourselves in the region of arbitrariness, as Gauss once said in reference to another matter; and if we will not approximate farther, but decide that equal weights shall be given to all the observations, if retained, we are compelled to find a criterion for rejecting some; for, of course, there frequently are certain observations which are prejudicial to the result, when retained having a weight equal to that of the best; so that the arithmetic mean would be nearer to the truth if they had never been made. Granting, then, that the middle course of weighting is not to be taken, and that all the observations are to have weight unity or zero, Pierce's criterion seems to me a very ingenious one; but still it is "arbitrary," viz. I presume others could be proposed having an equal claim to notice, as far as principle is concerned.

Mr. Stone's criterion, however, does not appear to be good, even among criteria, as it involves a principle which is, I think,

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unsound. I may mention that I should have alluded to Mr. Stone's paper in my Memoir had I known of its existence; but it is only recently that Prof. Asaph Hall, of Washington, called my attention to it by the expression of an opinion unfavourable to the criterion suggested therein. It is necessary to briefly recapitulate the nature of Mr. Stone's argument, but after what has gone before detailed comment will be unnecessary.

After alluding to a number of error-producing causes, such as accidentally touching the micrometer-heads or the declination-screw after the bisections have been made, entering erroneously the thermometer or barometer readings (for the refractions), &c., due to the observer's want of care, Mr. Stone remarks that "we have here a series of causes of errors entirely distinct from those which lead to the ordinary graduated errors," and proceeds, "For a given class of observations, and for a given observer, there must exist a number $n$, which expresses the average number of observations which that person makes with one mistake: $n$ may be defined as the modulus of carelessness. If, therefore, we find that value of $p$, which makes

$$\frac{2}{\sqrt{\pi}}\int_{\frac{p}{a}}^{\infty} e^{-y^2}\,dy^* = \frac{1}{n}$$

all larger values of $p$ are, with greater probability, to be attributed to mistakes than to the ordinary run of graduated errors. Larger values of error than $p$ must therefore be rejected."

In the first place it is to be remarked that the criterion cannot be intended as a practical one, as it is quite out of the question to make even a rough guess at the value of $n$; for (1) we do not know what sources of error are included, nor (2) within what limits the error is to be supposed to lie: it is to be "such a mistake as cannot be detected and corrected with perfect or almost perfect certainty;" and even were these two "unknowables" given, the determination of $n$ would be sufficiently arbitrary. In fact, Mr. Stone himself acknowledges that it is "perhaps impossible to determine the absolute value of $n$," but points out, as a set-off, that the limits of rejection vary only slightly for a large change in the value of $n$. It is pretty clear, therefore, that it would be a good deal easier to choose a limit of retention at once than to guess a value of $n$ and deduce one; so that I think I am quite right in regarding the criterion as a theoretical principle, and not as a merely practical rule: in fact, the author's own words are, "I have been led, on consideration of this subject, to a formula which embodies, in my opinion, the true grounds upon which my judgment rests when compelled to reject discordant observations."

The criterion seems to me most arbitrary, for why among all

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* $a$ is the reciprocal of the $h$ of this paper.

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the causes that can prejudice an observation should the carelessness (and only a particular kind of carelessness too) of the observer be the sole one to disqualify it from retention? Unless it could be shown that no cause could render an observation erroneous to a considerable* extent, except want of care on the observer's part, the principle fails; or, in other words, disturbing causes can arise in a variety of ways, and the criterion excludes from affecting the observations only such as have their origin in the carelessness of the observer. I am not here in any way straining the principle. Mr. Stone's own words are, "If this theory is correct, our rejection cannot be made except upon a direct admission of carelessness;" and this admission seems to me to pass sentence on the correctness of the criterion.

The principle comes to this, that the rule of least squares, as usually used, is competent to deal (*i. e.* without rejection) with errors over which the observer has no control, but is incapable of attending to errors that the observer could have avoided; and that a mathematical theory cannot draw so arbitrary a line is, I think, sufficiently evident *per se*, as it is hard to realise its discriminating between whether an error was due to the observer having written down 54 for 55, or to the fact of the observation having been made at a particularly unfavourable moment, when all the subsidiary errors happened to be of the same sign, and near their maxima. And this brings me to the only reasoning by which the principle could be defended, viz. by supposing that all the errors beyond the observer's control are continuous, and the errors due to carelessness, intermittent (viz. only operating once in $n$ observations). Admitting this (though I have great doubts about its truth), the basis of the criterion seems to me equally fallacious, for the following reasons:—

An observation is to be rejected when there is reason to believe that we obtain a result nearer to the truth by its rejection than by its retention; and whenever it is considered that the result will be improved thereby, the observation is to be rejected, no matter to what cause its (presumed) large deviation from the truth may be due.

Now, errors may arise in a perfectly legitimate way (that is to say, by the accumulation of smaller errors in the very way contemplated by the theory and independently of the observer's care) and yet their retention may be quite as disastrous as if they had been mistakes made by the observer; so that the distinction between the two cannot be a proper ground for discriminating

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* That is to say, considerable compared to the average magnitude of the continuous errors, but not greater than a continuous error can very well be. I purposely avoid using the term "graduated errors" to express those whose law of facility is continuous, as the adjective does not seem to me to convey to the mind the impression intended. Errors whose law of facility is continuous are here called *continuous* errors. An intermittent error, viz., one produced by a cause that operates only rarely, may have (and generally has) a continuous law of facility; but in this paper by "continuous" is generally to be understood continuous and non-intermittent.

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whether an observation is to be accepted or rejected. If, on Mr. Stone's theory, the observer never makes mistakes (or, rather, only makes large ones), no observations are ever to be rejected; and this merely amounts to a statement, in other words, of the principle noticed above, viz. that the rule of least squares, as it stands, looks after all the ordinary continuous errors, however large, so that they need not be considered, but that we must treat in some other way errors arising from an intermittent cause. The continuous errors are bound to look after themselves if the number of observations is infinite, but not otherwise; and in that case there is no distinction between the continuous and intermittent errors at all.

An example will illustrate the matter more clearly. Suppose that there were twenty coins, and that we tossed the whole twenty at one time, and, reckoning each head as $+1$ and each tail as $-1$, regarded the result as an error of observation (not as an observation itself) to which it bears a fair analogy. Let $m$ tosses of the whole twenty be made, then the error of the arithmetic mean of the observations (whatever they may be) is $\frac{E}{m}$, E being the algebraic sum of the errors, and this quantity becomes zero when $m$ is made infinite; but $m$ being finite, say $= 30$, the question is whether we should not do better to reject, say, one particular observation, the error of which appears as, say, 18 in the register, than to retain it. The error in question may have arisen in two ways, either by the circumstance of nineteen heads having appeared, or by the observer having made a mistake in counting or registering the number of heads that did appear. If Mr. Stone thought it more likely to be due to the latter cause, he would reject the observation, otherwise he would retain it. But, however it got there, it is equally prejudicial to the truth, and if the attainment of the truth is our object, it must be rejected solely on account of its departure from the average, not on account of what we think may have been its origin. No matter how the 18 originated, it is one of the observed errors, and whether it owes its existence to the theory of chance (as a run of luck) or to a careless observer, it should be equally rejected from (or retained in) a finite series of observations. If more and more observations were made, the large positive errors would more and more be balanced by the negative ones (relatively to $m$); but then, when the number of observations is much greater than the observer's modulus of carelessness, the small mistakes merge into ordinary errors (for an error-producing cause that only acts once in $n$ observations, and when it does act is subject to the law of facility $\phi(x)$, is then merely regarded as subject to the law $\frac{1}{n}\phi(x)$ where $\phi$ may be continuous or discontinuous).

I may also allude to another sentence in Mr. Stone's paper, viz. that in which he objects to a criterion proposed by Chauvenet for the rejection of a single observation, as "based on an erro-

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neous principle ; for in 2 $n$ such observations we ought reasonably to expect an error greater than or equal to $x$. In $n$ observations therefore we ought not to be surprised at the appearance of such an error, and certainly its appearance would be no ground for the assumption of some disturbing cause of error." Taking the words literally it is enough to reply that the question is not, how far we are surprised at such an error having been made, but whether it should be retained or not ; but the context shows that the real point involved is the distinction between avoidable and unavoidable errors, about which enough has been already said.

I must not be understood as expressing an opinion that the knowledge (if we had it) that an observer makes a small mistake in one out of every $n$ observations ought to go for nothing in the consideration of what observations should be rejected and what retained ; if we have such knowledge it is an additional datum, and ought to be used somehow so as to make us more willing to reject discordant observations : but what I think is, that Mr. Stone has invoked a false principle, and drawn a fanciful line of distinction between errors, in his desire to make use of it. *Ceteris paribus*, I much prefer a criterion, such as Pierce's, where the disqualification depends simply on the magnitude of the error. More large errors always do practically occur than the continuous-error theory (with the same $h$) would lead us to anticipate ; so that if we determine to give every observation either full weight or no weight, some ought to be rejected ; but whether under these circumstances it is possible to propose a criterion, independent of caprice, I feel very doubtful.

I hope I have made my meaning clear, viz. (1) that it seems to me that the proper treatment of observations is by weighting them after having found an approximate most probable result ; (2) that it is only because we reject this method, and insist on giving the observations full weight or no weight, that the necessity for a criterion arises ; and (3) that granting we are to use a criterion, the disqualification of an observation must depend solely upon the magnitude of its supposed deviation from the truth. Of course when I say observations are not in practice weighted, I leave out of consideration any weight arbitrarily assigned by the observer, and refer only to such as the theory of errors itself suggests.

There is some little ambiguity about the meaning of the words "disturbing cause ;" the idea which seems to be generally attached to them is any source different to those that give rise to the ordinary continuous errors ; but the following is an example of the sort of way they are sometimes used : an observation departs a good deal from what we believe to be the truth, therefore it is bad, and to have rendered it so, some disturbing cause must have been at work ; under these circumstances therefore the observation cannot be recognised as forming one of the series to which the rest belongs, but must be rejected. A statement of this kind must be taken as a truism, and then it in effect contains the definition of a disturbing cause, making it mean

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something or other which we presume does take place when the error is large. From this point of view therefore a run of luck is as much a disturbing cause as an observer's blunder, and so I regard it; but this is of course not the meaning that Mr. Stone and some others would assign.

I may, in conclusion, allude to what has been sometimes urged as an objection to the law $\frac{h}{\sqrt{\pi}} e^{-h^2 x^2}$, viz., that an error, however large, is possible. But such ought to be the case in an infinite number of observations; for, even if we could limit the continuous errors, we can set no bounds to the blunders. In practice we reject the mistakes directly we recognise them as such, but in an infinite series of observations, where everything is left to the theory, it applies to them as well as to any other class of errors. In the case of a mural-circle observation one might suppose that no one could by any carelessness make an error of more than 180°, nor can any one in reading the instrument, but the observer might register the observation as 1105° instead of 105°, when a greater error is introduced. It is, of course, not true that a really infinite error is possible, as the probability is proportional to $e^{-\infty^2} = 0$. Another point deserving notice is the statement usually received as self-evident, that positive and negative errors are equally probable. In certain conceivable cases this seems at first sight not to be the case; for, suppose one were to estimate the area of a field containing, say two acres, then it would be quite possible to say four acres, or even six, but absurd to say zero, or minus two (a case of this kind was pointed out to me by a pupil, in my lectures this term, as an exception to the rule). It is a sufficient justification of such an apparent exception to observe that an estimation (or guess) is not of the nature of an observation, as contemplated in the theory of errors; but still the analogy is close enough to make the discrepancy worth remark. The corresponding case in the theory of errors would be, if we had to measure a length so small that it was comparable with the errors of observation; a matter which would not in general be attempted. But if we were engaged with such a question, and $a$ and $b$ the abscissæ of the zero extremities of the length were observed, we should, of course, retain the values of $a - b$, whether positive or negative (viz. the mere fact of the length being negative, would be no ground *per se* for rejection).

*Cambridge;* 1873, *April* 2.

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## *On the Eclipses mentioned in the Anglo-Saxon Chronicle.*

By the Rev. S. J. Johnson.

Not having met with any work containing a description of the above, I have forwarded the following results to the Society